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The controversial effects of microfinance on child schooling: A retrospective approach*

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Abstract

Two crucial problems when research agencies or donors need to asses empirically the microfinance/children education nexus on already operating organizations are lack of availability of panel data and selection bias. We propose an original approach which tackles these problems by combining retrospective panel data, fixed effects and comparison between pre and post-treatment trends. The relative advantage of our approach vis-à-vis standard cross-sectional estimates (and even panels with just two observations repeated in time) is that it allows to analyse the progressive effects of microfinance on borrowers. With this respect our paper gives an answer to the widespread demand of impact methodologies required by regulators or by funding agencies which need to evaluate the current and past performance of existing institutions. We apply our approach to a sample of microfinance borrowers coming from two districts of Buenos Aires with different average income levels. By controlling for survivorship bias and heterogeneity in time invariant and time varying characteristics of respondents we find that years of credit history have a positive and significant effect on child schooling conditional to the borrower's standard of living and distance from school.

Keywords: child schooling, microfinance, retrospective data, impact evaluation.
JEL classification: D13, G20, I21, J22, J24, O12, O16, O18, O54

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1. Introduction

In a globally integrated world economy in which labor inputs and resources to finance physical capital investment are abundant and extremely mobile across countries a crucial constraint which prevents from achieving full output potential is lack of equal opportunities. For equal opportunities we mean the situation under which each individual, whatever her/his initial endowment of wealth, is allowed to develop her/his talent (and productive skills) by having access to education and credit or, from another perspective, the situation under which individual economic achievements are independent from inherited starting conditions. Since in any economy there is partial mismatch between those having productive ideas and those having the financial resources needed to fund them, the role of credit is fundamental and that of modern microfinance even more so. This is because the traditional banking system has serious limits in financing uncollateralized borrowers and therefore in allowing credit access to talented poor. The role of modern microfinance has been that of easing such access by replacing the role played by collateral on the borrowers' incentives with other mechanisms such as group lending (Banerjee, Besley and Guinnane, 1994; Besley and Coate, 1995; Ghatak, 1999) with joint liability, progressive individual loans and the threat of social sanctions (Wydick, 1999; Karlan, 2005a).

More in detail, the literature has defined four main channels (income, smoothing, gender and child labour demand) through which microfinance can affect child education (Maldonado and Gonzalez-Vega, 2008). First, if microfinance borrowers use their loans for financing projects which yield returns above the lending rate their income increases and, under the assumption of parental altruism (Basu and Van, 1998), the additional income may allow to overcome the threshold which triggers parents' decision to send their children to school. Consider however that this mechanism has its fragility since, if the project returns are delayed in time, income may fall and not rise in the short run due to the burden of loan repayments. Furthermore, the parental agency literature argues that parents may prefer to behave strategically not channelling the additional income on children education. In such case the impact of the income effect on child education will depend on the bargaining process between parents and children (Basu, 2002, Moehling, 2006).

The second channel argues that if loans assist consumption smoothing (Pitt and Khandker, 1998; Khandker, 2005; Islam, 2007) microfinance borrowers should not need to smooth consumption by withdrawing children from school (Kanbur and Squire, 2001).

The third channel states that microfinance promotes children education when, as in many cases, microfinance borrowers are mainly women since the latter have relatively stronger preferences for education than men (Pitt and Khandker 1998; Behrman and Rosenzweig 2002; Thomas, 1990; Behrman and Rosenzweig, 2002; Sallee, 2001). Consider however that this channel works only

when the formal loan entitlement coincides with an effective shift of power toward women within the family.

Finally, the fourth channel (child labor demand effect) identifies an unequivocally negative impact of microfinance on children education. If microfinance leads to an expansion of household productive activity, and if children can usefully be employed in it, the loan may increase the opportunity cost of sending children to school. The same result can be obtained if the loan leads to an increase in hours worked by parents therefore making children more necessary to perform household chores. In both cases credit access may increase demand of child labor thereby reducing child schooling (Psacharopoulos, 1997; Jensen and Nielsen, 1997; Patrinos and Psacharopoulos, 1997; Grootaert and Patrinos, 1999; Trigueros, 2002).[1]

Given these conflicting effects in the relationship between microfinance and child labour, it is of foremost importance to develop sound empirical research verifying whether microfinance performs the task of promoting equal opportunities through easier access to education for borrowers' children.

Surprisingly there are not many papers looking at the general issue of microfinance and children wellbeing and very few of them look explicitly at children education. This is probably not due to lack of interest but to the daunting task of developing a convincing impact analysis which overcomes methodological problems of selection bias, particularly severe in microfinance studies.[2] Among the existing papers negative associations between child labor and access to credit are found by Dehejia and Gatti (2005) and Jacoby and Skoufias (1997)[3]. Yamauchi (2007) finds that investment in household enterprise does not necessarily eliminate child labour or promote children's education in rural Indonesia, while Hazarika and Sarangi (2008) report that, in rural Malawi, children tend to work more in households that have access to microcredit.[4] In other

[1] Consider however that in many cases the increased demand for child labor may lead to forms of part time work and school, thereby not affecting directly schooling choices even though such option has been shown to affect negatively children schooling performance (Edmonds, 2007).

[2] In microfinance selection bias is embedded in the screening process of lenders who have to select most talented borrowers with profitable projects. A successful screening process is therefore automatically expected to produce heterogeneity between accepted and excluded loan applicants.

[3] Dehejia and Gatti (2005) use cross-country panel data and find a negative association between financial development and child labor. Such effect is showed to be particularly stronger in developing countries because of higher income variability. The authors conclude that credit markets allow households and firms to smooth shocks in the economy. Jacoby and Skoufias (1997) examine how child school attendance reacts to seasonal fluctuations in the rural households' income. Their conclusion is that unanticipated income shocks significantly affect children's school attendance and therefore uninsured households withdraw children from school in response to unanticipated income shocks, but not in response to anticipated shocks.

[4] Hazarika and Sarangi (2008) find that, in the season of higher labor demand, children's propensity to work is increasing in household access to microcredit (measured as self-assessed credit limits at microcredit organizations) in rural Malawi. Their school attendance is however not reduced, suggesting that increased

papers the nexus is shown to depend on various factors such as the type of microfinance institution (hereon MFI) (Pitt and Khandker, 1998), the type of investment and borrower activity.[5]

Our paper aims to provide an original contribution to this literature by testing the impact of microfinance on child education with a novel methodology. The originality of our approach is in the creation of retrospective panel data and in the use of fixed effects and pre-formation trends in estimates where the *length-of-access effect* is estimated on a sample of microfinance borrowers. In our opinion, the combination of these elements aims to solve two main problems common to many impact studies: i) selection bias when the researcher want to analyse the performance of an already existing organization and it is impossible to run randomized experiments. In this sense our paper gives an answer to the widespread demand of impact methodologies required by regulators or by funding agencies which need to evaluate the performance of existing institutions; ii) dynamic analysis when repeated observations in time require too much time and costs to be collected and in many cases are not available because data collection was not planned ex ante. With respect to this point our approach allows to explore an otherwise fundamental and unobservable effect of the impact of our treatment (microfinance), that is, its progressive effect across years for the same individual.

The rest of the paper is organized as follows. In the second section we describe the characteristics of the microfinance institution under scrutiny. In the third section we explain the sampling procedure and in the fourth we illustrate the characteristics of our database, commenting some descriptive findings. In the fifth section we explain our econometric approach and robustness checks and discuss the obtained findings. The sixth section concludes.

2. The main features of the MF institution under scrutiny

> *"[...] The help we received from Protagonizar was enormous. I felt that not everything was lost. On some occasions we tried to get a bank loan but they asked for a credit card and wages receipt; impossible. Here instead, we go with our word, they believe and trust us. This is beautiful and I feel we are not alone [...]".* [6]

Protagonizar is a young and small microfinance organization with six years of life and more than 3,000 disbursed uncollateralised loans. It is a non-profit foundation operating in Argentina in the

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child labour - mainly in the form of household chores - reduces leisure rather than schooling.

[5] Wydick (1999) finds that the relationship between child education and microfinance is not univocal and reports that the probability of child work is higher if the loan finances capital equipment and not working capital investment. Maldonaldo and Gonzalez-Vega (2008) find that households demand more child labour if they cultivate land and operate labour-intensive microenterprises.

[6] Extracted from the "*microentrepreneurs' stories*" section of the *Protagonizar* handbook (2005).

second belt of Gran Buenos Aires (area of San Miguel) with small businesses (bakeries, textile enterprises, beehives or basketworks) of poor microentrepreneurs. *Protagonizar* performs its activity with credit agencies located in the three "villas" (densely populated sub-urban areas) of *Santa Brigida, Barrio Mitre* and *Villa de Mayo*.

The organization claims that its competitive advantages are the low operative costs (modest facilities, low installation and reduced functioning costs), the reduced distance from borrowers and the time dedicated in counselling and assisting them by the bank mixed staff composed by volunteers and paid professional staff members.

An interesting feature of *Protagonizar* is that the organization moved in the opposite direction with respect to the well known Grameen case, since it started from *staggered individual credits* and moved more recently to a *group lending mechanism* with *full joint liability*.

The old *staggered individual credit* approach created a group of three entrepreneurs with independent projects giving credit sequentially to each member conditional to the repayment of the previous borrower. The *Protagonizar's group lending* approach hinges on the creation of a group of 4-6 individuals to which money is given simultaneously. Group members have full joint liability. One of the group members, appointed group coordinator, is in charge of receiving the money from Protagonizar, distributing it among group members and collecting payments on behalf of the lender.

Eligibility criteria for group lenders are as follows. Borrowers are required *i)* to have at least six months of entrepreneurial experience, *ii)* not to be relative *iii)* to be located at no more than three blocks of distance from each other (a rule which aims to ease peer monitoring) and, *iv)* to have different business activities in order to diversify risk within the group. Among such activities only one street vendor per group is allowed. The microfinance institution charges 5% monthly[7] over the debt balance for both (staggered individual and group) loans. [8] Repayments take place on weekly basis.

A specificity of the *Protagonizar* group lending approach is its three-sided screening process. The first two checks are represented by the MF organization screening activity and other bank

[7] Real interest rates seem high if we consider official, but less so if we consider unofficial inflation rates. Consider in fact that Argentinean poverty lines are considered grossly undervalued due to a downward bias in computing domestic inflation. One of the main independent research centers, Ecolatina, estimates that prices rose 65 percent from Dec. 1, 2006, to July 31, 2009, compared with the 20 percent increase calculated by the statistical institute (to follow this debate see:
i) http://www.bloomberg.com/apps/news?pid=newsarchive&sid=aKQUiLozzZko and ii)
http://www.bloomberg.com/apps/news?pid=newsarchive&sid=a5joiySC_mXc.

[8] The average lending rate charged by moneylenders in the three villas is around 50 percent monthly.

borrowers' evaluation of the payment capacity of the prospective client. The third check is the group lending mechanism. The latter is expected to induce assortative matching (Armendariz and Morduch, 2005) since, for groupmate-neighbours, trust on borrower's creditworthiness has pecuniary consequences and is demonstrated by accepting to create a group with her under joint liability.

During the screening process would be borrowers are visited by credit advisors to which they provide socio-demographic and business information by filling a standardized form. In a second step credit counsellors/advisors are asked to assess their credit capacity. The latter then formulate their proposal to the Credit Committee. If the lending decision is taken counsellors/advisors also perform monitoring activities with post-credit visits on weekly basis.

Most relevant to the object of our research, Protagonizar has a neutral attitude toward child schooling. Its approach is targeted to support with financial resources borrower's business and growth in economic opportunities while the goal of child schooling is neither in its operating activity in the field nor in its declared principles. This neutral stance reduces the potential confusion between schooling effects generated by the need to comply ex ante with the MFI's standards and those caused by the ex post effect of the microfinance loan.

3. The research design

Given the impossibility of running randomized control trials, we implement an ex-post impact evaluation based on quasi-experimental data. From June to September 2009 a questionnaire has been delivered to 360 micro-entrepreneurs located in proximity of the three agencies of Protagonizar (Santa Brigida, Barrio Mitre and Villa de Mayo) by two teams composed by one researcher and one field assistant each.[9]

A treatment group of 150 clients (in equal proportion from *Barrio Mitre* and *Santa Brigida*) is formed randomly from a list of all MFI borrowers by keeping into account the heterogeneous seniority of the membership.[10]

As a control sample, from the three areas of interest we randomly interview 150 *eligible non participants* micro-entrepreneurs who were not borrowers (neither of Protagonizar nor of any other MFI) at the moment of the interview.[11]

[9] The questionnaire is omitted for reasons of space but is available from the authors upon request.
[10] Borrowers' seniority is evaluated according to their credit-cycle.

In addition to the treatment and control groups, we also create a sample of 60 Protagonizar's former borrowers who dropped out from the program.[12]

By choosing members of the control group according to eligibility criteria we are able to reduce the potential heterogeneity between MFI and non-MFI types and thus the *selection bias*. Moreover, the inclusion of drop-outs is aimed to tackle the effects of the *survivorship bias* on our estimates (Karlan and Alexander-Tedeschi, 2009).

4. Database and descriptive findings

A first descriptive element which gives us an idea of the local standard of living and of the distance of the respondents from the poverty line is the monthly mean and median household income in the whole sample which amounts to 4,096 and 3,000 pesos respectively. This implies that households live on average with around 136,53 pesos per day. Since the median number of members in the household is around 4, interviewed individuals live on with roughly 34.13 pesos/day, that is around 16.78 PPP US$/day using the country's implied PPP factor computed by IMF in 2009.[13]

Average schooling years of the respondent in the sample are quite low (8.4 years) and those of the partner even lower (5.8 years) (Table 1). Average total productivity (considering main and secondary jobs) is around 17 pesos per hour.

Microfinance clients repay on average 108 pesos each month, that is, 27 percent of median income. In spite of it around 20 percent of income is saved. Finally, MF borrowers' productivity is 21 pesos per hour worked against 16 pesos of eligible non-participants (again the difference in means is not significant at 5 percent).

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[11] Individuals who are not clients at the moment of the interview might instead have asked and received a loan in the last 20 years, the time span we consider for the retrospective panel. However, since Protagonizar is the first and the only organization providing micro-loans in the three villages, if (present) eligible non-participants asked for a loan in the past they must have received it from formal banks or moneylenders (but not from other MFIs). Such an event would, however, not change the core of our analysis about the dynamic impact of microfinance (specifically, the micro-financial services provided by Protagonizar) on children's education.

[12] We selected a number of dropouts from each area which is proportional to the historical exit rates of borrowers from the organization.

[13] During the survey period (July-Sept. 2009), the average malnutrition and poverty thresholds were set by the INDEC (National Statistical Agency of Argentina) at 4.88 and 11.04 pesos/day respectively, which are in turn equivalent to 3.84 and 8.70 PPP –US$ according the PPP country's factor evaluated by the World Bank in 2005. When considering the country's implied PPP factor in 2009 (US$ 2.033, source: IMF), both the malnutrition and poverty lines fall to 2.40 and 5.43 PPP-US$ per day respectively. However, if we correct these lines for the unofficial and more realistic inflation rates discussed in footnote 7, *Protagonizar* borrowers are much closer to poverty.

To go beyond overall sample averages we present descriptive statistics dividing the sample in three groups in Table 2 (clients, eligible non participants and dropouts) and in three groups in Table 3 (respondents living in *Barrio Mitre*, *S. Brigida* and *Villa de Mayo*).[14]

In Table 2 we observe that clients have higher mean household income than eligible non-participants (4,982 against 3,662 pesos) which have in turn higher income than dropouts (2,958 pesos). However, the difference between dropouts and clients is significant at 5 percent while that between them and eligible non-participants is not. Ranking and significance are substantially unchanged if we consider median household income. Such findings document that individuals who drop-out are likely to belong to such a group due to some form of underperformance. The same ranking can be observed when we look at productivity, [15] highest for clients (20.60 pesos per hour worked) and lowest for dropouts (13.18 pesos), with eligible non-participants in the middle (15.75 pesos). The three groups are however substantially homogeneous in terms of demographic variables (household size, respondent education and age). Finally, clients have higher job experience and save more even though the differences among groups are not significant in this case.

In table 3 we observe that the geographical breakdown also matters. Individuals in Barrio Mitre are relatively poorer than those in S. Brigida ---average household income is respectively 3,677 and 4,419 pesos. This implies that villagers from Barrio Mitre live on with 30.64 pesos/day (15.07 PPP-US$) whereas those of S. Brigida with 36.85 pesos/day (18.11 PPP-US$)[16].

There is also a marked difference between the two areas in terms of productivity (20.08 pesos in S. Brigida and 15.45 pesos in Barrio Mitre) and savings (238.9 and 178.9 pesos respectively). We expect that such differences in income, savings and productivity may affect the impact of microfinance on the probability of schooling of respondents' children. Households in Villa de Mayo seems to perform better than households in Mitre but slightly worse than those in S. Brigida in terms of income and productivity (45.35 and 35.51 pesos respectively); their monthly savings (29.18 pesos) are however lower than those of the households in the other villages.[17]

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[14] We include a third village (Villa de Mayo) in which Protagonizar activity has just started and there are no treatment group observations (MFI borrowers). This is typically done in impact studies in order to reduce the noise generating potential spill-over effects from treatment to control group in the two other villages. The econometric results of the paper presented in section 5 are however robust in a check in which we exclude respondents of Villa de Mayo from the control sample. Results are omitted here for reasons of space and available upon request.

[15] Measured as the ratio between respondent and her partner's monthly income (from all their activities) and the hours they spend in each activity.

[16] See footnote 7 and 13 for a discussion on poverty lines measurements in Argentina.

[17] A further breakdown of descriptive statistics by geographical location and interviewed status is provided in tables A1-A3 of the appendix.

5.1 Econometric specifications

Two serious problems in impact analyses on development projects on existing organizations are the impossibility of running randomized experiments and the lack of time series data. More commonly researchers dispose of a cross-section or of just two observations (before and after a given treatment) for each individual. A possibility to overcome these limits is the reconstruction of detailed time series from a cross-sectional survey with retrospective data.

The retrospective reconstruction of time series is based on past information required from respondents in cross-sectional surveys and commonly adopted in the literature when costs of collecting data across time are too high or the researchers need to evaluate an economic phenomenon for which this information is not available. Among various examples see Peters (1988), McIntosh et al. (2010) and Becchetti and Castriota (2009)[18].

The approach is reliable when past information demanded does not require unreasonable mnemonic effort and hinges on the identification of simple memorable events. As a matter of fact the three empirical contributions mentioned above ask respondents to identify years of events such as divorces and remarriages (Peters, 1988), house restructuring decisions (McIntosh et al., 2010) and schooling years and age of children (as in our case). In discussing such methodology McIntosh et al. (2010) include among memorable events major diseases, deaths, school enrolments, and major asset purchases, while consider changes in profits and revenues among those which are more difficult to remember with precision.

An important validating check for this approach is provided by Peters (1988) who compares the accuracy of retrospective information provided by respondents to a cross-sectional survey with panel data collected across time and demonstrates that both sources of data give substantially the same results when estimating hazard rates of divorce and remarriage. Finally, consider as well that retrospective data present some advantages even with respect to standard panel data since they do not suffer from attrition bias problems.

The use of retrospective data fits well the object of our inquiry. The information required from respondents to build the retrospective information is relatively easy to remember. We demand the

[18] Other examples of the use of retrospective data are provided by *i)* Garces at al. (2002), who use PSID data with the addition of retrospective questions on early childhood education in order to assess the impact of a public preschool program for disadvantaged children; *ii)* Smith (2009), who examines impacts of childhood health on socioeconomic status outcomes observed during adulthood relying on retrospective self-evaluations of the general state of one's health and *iii)* Ilahi et al. (2000) who, using unique retrospective data from Brazil, explore the relationship between child labor, future adult earnings and poverty status.

number of children in the family, their age and the number of school years they have attended. We also verify whether there have been exits and reentries in the schooling record, as well as repetitions. Based on the use of retrospective data we build time series of schooling decisions for each children in the respondent household in a 20-year time horizon.

We therefore test the effect of years of credit history (affiliation)[19] with the MFI on schooling decision using a logit fixed effect on the following specification: [20]

$$School_{ijt} = \alpha_0 + \alpha_1 NChildren_{jt} + \alpha_2 JobExperience_{jt} + \sum_{m=3}^{7} \alpha_m ParentageCohorts_{jt} + \alpha_8 PreAffTrend_{jt} + \\ + \alpha_9 Childage_{ijt} + \alpha_{10} AffilYears_{jt} + \sum_{l} \alpha_l DYears_l + \upsilon_i + \varepsilon_{ijt} \quad (1)$$

where ($School_{ijt}$) is a dummy taking value of one if the *i*-th children of the *j*-th family went to school in the year *t* and zero otherwise. Among socio-demographic variables we introduced those for which theoretical and empirical literature on child schooling has extensively demonstrated relevance and significance on child schooling decisions (see among others, Edmonds, 2007, Islam and Choe, 2009 and Maldonado and Gonzalez-Vega, 2008). $NChildren_{jt}$ is the number of children in the family *j* at time *t*, *JobExperience* is the respondent's job seniority (number of years worked in the current (time of the survey) activity), *ParentageCohorts* are the respondents' age categories,[21] *PreAffTrend* is a (pre-affiliation) trend variable measuring the number of years for family *j* before becoming Protagonizar's borrower, *Childage* is child's age, *DYears* are time dummies (1989 is the omitted benchmark), *AffilYears* are the years of affiliation (years of uninterrupted lending relationship) of family *j* at time *t* for client and dropout samples.

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[19] We define for simplicity years of affiliation as the time length of uninterrupted relationship with the lender (i.e. the time distance between the first loan received and the year of the survey for current borrowers with subsequent credit cycles).

[20] The approach is also known in the econometric literature as the *conditional likelihood approach* and allows to "difference out" individual effects in non-linear panels through a transformation that is the analogue of first differencing in the linear case. The basic idea is to consider the likelihood conditional on sufficient statistics for the individual effects (that is, the individual specific mean or, equivalently, the individual specific frequency in case of a logit link). Then, conditioning on the individual fixed effects, choices in the *T*-periods are independent. In this setting, a standard logit model is then obtained where the probability of the binary outcome does not longer depend on individual effects (which have been differentiated out) and where changes in the regressors between the *T*-periods allow to predict changes in the dependent variable. See Andersen (1970) and Chamberlain (1980) for a more detailed description of the technique. The main advantage of this approach is that neither distributional nor independence assumptions on the unobservable individual effects are required. However, this comes at the cost of having a sufficient number of units for which a change of state is observed; because of this requirement, only a small fraction of the sample might be used for the estimation.

[21] In order to avoid perfect multicollinearity which would arise from including year effects, respondent parent age and fixed effects we create dummies for any two year interval and dummies for parent age categories. We split the respondents' age into five cohort dummies: 29-33, 34-38, 39-43, 44-48 and over 49 years old. The omitted reference age category is 0-29.

In different specification of equation (1) (see Table 4) affiliation years are interacted with a village dummy (*S.Brigida*) and a distance dummy, *Distant*, equal to 1 if the child i lives above the median distance far from the school. We use such interactions in order to catch the progressive microfinance effect on wealthier borrower (the ones living in S. Brigida) and on families with higher indirect costs of schooling (proxied by the distance from the school).

With regard to child fixed effects υ_i they incorporate (but do not allow to measure separately) important time invariant effects such as those of gender, parental education and district location. The specific impact of these variables will be evaluated with different estimating techniques in our robustness checks.

Note that we do not have data on household income, a variable which is often impossible to track or is highly imperfectly measured due to interview bias. As a consequence many papers use proxies which are more easily measurable and less subject to bias such as parental education.[22] In addition to it, we use here years of experience in the current job which is another important proxy under the reasonable assumption of learning by doing and tenure effects on income.

Parental age is introduced here to measure something different from parental education (to which it is also correlated). Older parents may be less willing to send their children to school because they are linked to less schooling oriented traditions or because their age increases the need of being supported by children in their job activity.

The inclusion of the pre-treatment trend variable allows us to evaluate the effect of affiliation years on the treatment group by looking at the trend before and after the beginning of the bank-borrower relationship.

When estimated on the overall sample, our specification therefore allows us to compare outcomes of the treatment group (borrowers) with the control group represented by eligible non-borrowers[23] by assuming that the two groups have homogeneous characteristics. We control for heterogeneity between the two groups determined by (children better than family) time invariant characteristics with child fixed effect, while we take into account time varying heterogeneity with comparison of pre-formation and post formation trends.

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[22] In a cross-sectional estimate we find that job experience and parental education account for a relevant part of the variability of current respondent's income. The estimate is omitted for reasons of space and available upon request.

[23] Eligibility criteria in Protagonizar group lending require that borrowers *i)* have a minimum six month enterprise experience, *ii)* are not relative but *iii)* live at a maximum of three blocks of distance from each other (a rule which facilitates peer monitoring) and, in order to diversify risk within the group, *iv)* have different business activities (only one street vendor per group is allowed). We apply criteria i) and iii) to create the control group in our sample.

5.2 Econometric findings

Table 4 presents results from fixed effect estimates. In the first and second column the model is estimated on the whole sample and on the subsample of microfinance borrowers plus dropouts in order to evaluate the microfinance impact after controlling for survivorship bias (Karlan and Alexander-Tedeschi, 2009).

Such estimate also allows to tackle more effectively the problem of heterogeneity between treatment and control group and the related selection bias. As it is well known, even though we select local eligible non-borrowers in order to enhance homogeneity between treatment and control group, it is not possible in principle to exclude self selection effects, that is, ex ante factors correlated both with individual productivity and the decision to become borrowers. This problem would widen the gap between the first best comparison with the counterfactual (what would have been the child schooling performance of the borrower's offspring if he had not borrowed from the MFI) and our approach. The estimate excluding the control group eliminates such problem and isolates the dynamic effect of the borrower-bank relationship on our dependent variable.

Consistently with what expected we find a significant negative relationship between parental age and child schooling, with a positive effect of parents below 43. Child age is negative as expected.[24] The time varying regressor measuring parent's years of experience in the job they are still performing at the time of the survey is positive and significant. Since it is reasonable to assume that, due to learning on the job and work tenure effects, the variable is a proxy of the respondent income, such a finding probably captures part of the positive effect of the unobserved income variable on child schooling.

Note that fixed effects incorporate the impact of all time invariant drivers of child schooling. As a consequence we cannot detect in this estimate the separate effect of child gender and parental education (invariant in our sample). Another proxy of income (parent education) is therefore incorporated in fixed effects. The unique counterintuitive result we have is that of the positive effect of the number of children on child schooling. Consider however that in fixed effect estimates this variable captures only within effects, that is the impact of a new birth on child schooling.

[24] On the effect of children's age on education attainments we expect that, the older the child, the more likely that she/he will show an education gap. Such a result is confirmed by Maldonado and Gonzalez-Vega (2008) and Islam and Choe (2009) who find children at primary-school age to have a higher enrolment rate compared to their older siblings, the latter being more likely to drop out from school and go to work.

The effect of affiliation years is not significant in this first estimate leading us to infer that the various effects (income, risk management, gender and child labour demand) compensate each other.

As discussed above, to understand more about what happens beyond the average overall sample effect we create two slope dummies interacting affiliation years with residence in the wealthier S. Brigida district and distance from school above the median distance in the sample. The rationale for the creation of this two variables is that: i) if the luxury axiom (Basu and Van, 1998) holds (see introduction), with higher income the child schooling effect should prevail; [25] ii) families who are more distant from school pay higher (pecuniary or just opportunity) cost of transport, especially in areas such as those included in our survey where problems of criminality verified by our interviewers are very serious (and children must presumably be accompanied by an adult, especially if they are far from school).[26]

In columns 3 and 4 (Table 4) we introduce only the district slope dummy and find that the effect of location on S. Brigida is positive and significant both in the overall sample and in the estimates with borrowers and dropouts only. In columns 5 and 6 we introduce the distance dummy and find that its effect is positive and significant as well. When in columns 7 and 8 we introduce both variables we find that both distance and district slope dummies are positive and significant when jointly considered. In order to evaluate more clearly the interaction effect of location in S. Brigida and distance from school we estimate an additional specification in which a dummy for respondents with both characteristics is interacted with affiliation years and compared with the benchmark affiliation year effect. This slope dummy is significant and strong in magnitude (columns 9 and 10).

An important parallel result which reinforces our main findings is the lack of significance of the pre-treatment trend which clearly identifies a structural break in the schooling performance around the beginning of the bank-borrower relationship. This result documents that the dynamic effect of the bank-borrower relationship does not depend on a spurious positive child schooling trend, preexisting to the affiliation date.

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[25] A related interpretation is that current productivity and household income of borrowers may be a proxy of past values of the two variables. In this respect descriptive evidence at Table 3 shows a significant difference between borrowers in S. Brigida and Mitre in the year of the survey. The higher productivity and standard of living of the former may have generated enough savings to increase school attendance of the children in the household during the lending period.

[26] To quote just an example the local team supporting our researchers refused to accompany them in Mitre at late morning and afternoon for the danger of meeting criminals or drug addicts.

Since affiliation is non synchronous (it occurs at different time for each borrower) it is difficult to interpret a difference in pre-treatment (*PreAffTrend*) and post-treatment (*AffilYears*) trend effects on child education as due to other unmeasured concurring factors. One possibility is that the effect is not due to the treatment but to requirements that the organization poses on would be borrowers in terms of child education (i.e. a precondition for being financed by the MF is that borrowers send their children to school). However, as we documented in section 2, *Protagonizar* is neutral (does not take any position) with respect to the child schooling issue. Furthermore, if a test on a dichotomous treatment effect may be subject to this observational equivalence, this is not the case of a gradual impact which grows with affiliation years. The precondition hypothesis would not explain why the education outcome improves across years even after the beginning of the relationship with the MFI.

Overall our findings document that the effect of microfinance on child schooling is positive and significant only conditionally to geographical location (in S. Brigida) and distance from school of borrowers. Given the difference in standard of living (and current sample income and productivity)[27] between the two areas in which Protagonizar operates since more time, we can interpreted results by arguing that borrowers can be divided into four groups according to these two crucial variables (S. Brigida and Mitre residents close and far from school). Only one of these groups seems close to the luxury axiom threshold so to experience the stronger benefits from microfinance loans in terms of child schooling.

Consider as well that affiliation results (when estimated in the overall sample) can be explained neither by heterogeneity in time invariant characteristics between treatment and control sample (captured by fixed effects) nor by heterogeneity in a time variant factor which ensured progress in child education even before the "affiliation period" (the lack of significance of the pre-affiliation trend).

Finally, our child schooling results can be hardly related to a pro-schooling stance of the microfinance organization. As explained when describing the organization, its attitude toward this issue is absolutely neutral. Even if it were not, so the difference between pre and post treatment schooling trends documents that there are no traces of pre-formation attitude of future borrowers to conform to a child schooling prerequisite by the organization. The gradual positive effects observed only for a subgroup of borrowers also confirm that there is not a uniform effective overimposed schooling requirement.

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[27] Given the types of activities of *Protagonizar* borrowers and the limited reach of their potential market we may reasonably assume that local standard of living is the crucial variable affecting local demand and thereby driving income and productivity of most borrowers whose activities have mainly local customers.

5.3 Robustness checks

Results from the previous section highlight a positive effect of affiliation years on the probability of child schooling for i) borrowers living in S. Brigida ii) borrowers more distant from schools iii) borrowers of S. Brigida located more distant from schools.

A limit of our dependent variable may arise is that within variation (switches from 0 to 1 or vice versa) is limited. In our sample switches, that is changes in the dependent variable from t-1 to t, amount to 10 percent of total observations. The number is not so limited but however suggests us to perform further robustness checks.

First, we propose a simple logit pooled estimate in order to consider a larger number of observations and disentangle the effects of time invariant characteristics (such as gender and parent education) which were incorporated in fixed effects in the base estimate. The baseline equation we consider is the following:

$$\begin{aligned} School_{ijt} = {} & \alpha_0 + \alpha_1 NChildren_{jt} + \alpha_2 JobExperience_{jt} + \alpha_3 Parentage_{jt} + \alpha_4 RespEducation_{jt} \\ & + \alpha_5 PartnerEducation_{jt} + \alpha_6 PreAffTrend_{jt} + \alpha_7 Male_{ijt} + \alpha_8 Childage_{ijt} \\ & + \alpha_9 AffilYears_{jt} + \sum_l \alpha_l DYears_l + \varepsilon_{ijt} \end{aligned} \quad (2)$$

Regressors in the pooled logit estimate are therefore the same as those in the fixed effect estimate with the addition of *Male*, a dummy taking value of one if the child is male and zero otherwise, *RespEducation* (respondent's schooling years) and *PartnerEducation* (schooling years of the respondent's partner). Parental education is an important factor which is expected to have a positive and significant effect. This is due to the fact that the higher stock of human capital in the family not only generates higher income if "returns to schooling" work but also a more optimistic parental perspective on the benefit of education for their children (Maldonado and Gonzalez-Vega, 2008)

Problems of multicollinearity are greatly reduced with the omission of fixed effects so that we can replace parent age categories with parent age. Pooled logit estimates allow us to identify a positive education and gender (male) effect. The gender effect is positive and consistent with what expected in the literature about girl education to be less valued than the boys' education so that girls should exhibit a wider education gap (Table 5).[28]

[28] In this respect, Islam and Choe (2009) find that girls elder than 13 years (in the control group) tend to have a lower enrolment rate, whereas no differences between girls and boys in their educational achievements are found in Maldonado and Gonzalez-Vega (2008). In addition, as commented by Edmonds (2007), data from

The positive effects of the interaction of distance from school and S.Brigida location with years of MF-borrower relationship is confirmed in these estimates. Note however that, with pooled logit estimates, the baseline effect (*AffilYears*) - not significant in the base estimate - becomes negative, even though weakly so in the subsample of borrowers and dropouts. The comparison of this point with the fixed effect findings suggests the presence of some heterogeneity between borrowers and eligible non-borrowers with the first having time invariant characteristics which make them less prone to child schooling. An interpretation is that eligible non-borrowers are in healthier financial conditions or are in jobs with rosier perspectives (and this can motivate their non borrower status).

The pooled logit estimation does not account for either the panel structure of the data or unobservable child-specific characteristics that might be correlated with the outcome variable (school attendance). Hence we re-estimate specifications 7-10 of table 4 with different approaches, namely i) logit child-random effects, ii) 3-level logit random effects iii) using *Education Gap* as dependent variable to address the scarcity of switches in the *School* dummy. The following sub-sections clarify each of the different robustness checks we use. Results are consistent with the previous ones, confirming the positive dynamic effect of microfinance on child-schooling only for the sub-sample of villagers from the richer area (S. Brigida) and for those who face higher transport costs since located distant from the schools.

a) Child-Random effects

We re-estimate the baseline model with random-effect logistic model for specification 7-10 (table 4). Equation 1 then becomes:

$$\begin{aligned} School_{ijt} = {} & \alpha_0 + \alpha_1 NChildren_{jt} + \alpha_2 JobExperience_{jt} + \sum_{m=3}^{7} \alpha_m ParentageCohorts_{jt} + \\ & + \alpha_8 RespEducation_{jt} + \alpha_9 PartnerEducation_{jt} + \alpha_{10} PreAffTrend_{jt} + \alpha_{11} Male_{ijt} + \\ & + \alpha_{12} Childage_{ijt} + \alpha_{13} AffilYears_{jt} + \sum_{l} \alpha_l DYears_l + \upsilon_i + \varepsilon_{ijt} \end{aligned} \quad (3)$$

where υ_i are the child-specific unobserved random intercepts assumed to be normally distributed with zero-mean and variance σ_v and ε_{ijt} are the zero-mean and unit-variance normally distributed error terms. A stronger assumption is typically needed for the estimation of non-linear panel random effects models, namely that υ_i and ε_{ijt} are independent (not just mean independent). Individual random effects are then "integrated out" usually using a quadrature method.

UNICEF's Multiple Indicator Cluster Surveys show that there is a sizeable increase in participation rates in market and domestic work for males at age 12, while girls experience discrete jumps at age 8, 10, and 12. The increase at age 8 for girls appears to be most dramatic in domestic work, whereas most of the increase at age 10 and 12 for girls is in market work.

Results are reported in the first two columns of Tables 6 and 7. As in the previous pooled logit estimation, we find a positive impact of parental education but a negative effect of the length of MF-affiliation on the probability of child's school attendance. However, the latter negative effect is counterbalanced by a positive and significant impact of MF-affiliation when borrowers live in S. Brigida (interaction *AffilYears*Sbrigida*) and when they are located far from the school (interaction *AffilYears*Distant*) as shown in Table 6, columns 1 and 2.

When we consider as explanatory variables the length of MF-affiliation (*AffilYears*) and its interaction with the borrowers living in S. Brigida that are more distant from the school (*AffilYears*Sbrigida*Distant*), only the latter variable shows a significant and strong positive coefficient (Table 7, columns 1 and 2). The findings are also robust to the sample split.[29]

b) *Three-Level Random Effects*

In order to control for child *and* family unobservable heterogeneous characteristics, we re-estimate equation 3 using a three-level random logistic intercept model for which in *t* time occasions (first level) we observe *i* children (second level) nested within *j* families (third level). Hence equation (3) becomes:

$$\begin{aligned} School_{ijt} = {} & \alpha_0 + \alpha_1 NChildren_{jt} + \alpha_2 JobExperience_{jt} + \sum_{m=3}^{7} \alpha_m ParentageCohorts_{jt} + \\ & + \alpha_8 RespEducation_{jt} + \alpha_9 PartnerEducation_{jt} + \alpha_{10} PreAffTrend_{jt} + \alpha_{11} Male_{ijt} + \\ & + \alpha_{12} Childage_{ijt} + \alpha_{13} AffilYears_{jt} + \sum_{l} \alpha_l DYears_l + \upsilon_i + \varphi_j + \varepsilon_{ijt} \end{aligned} \qquad (4)$$

where υ_i and φ_j are respectively the child and family-specific unobserved random intercepts and ε_{ijt} are the idiosyncratic error terms. The same distributional and independence assumptions made in the random effect model previously commented extends also here, both on υ_i and φ_j. Such approach allows us to control separately for child and family heterogeneous and unobservable characteristics that might lead to biased estimates of MF-affiliation effect.

Results are very similar to those we get from the previous model (chid-random effects) and are reported in columns 3-4 of Tables 6 and 7.

[29] Consistently with previous results, we also find in this specification that the presence of younger parents positively affect the probability for a child to be at school whereas this probability declines when the child's age increases.

c) *Education Gap*

To account for the scarcity of changes in the dummy dependent variable $School_{ijt}$ in the baseline fixed effect model, we construct an alternative child-varying schooling variable (*Education Gap)*.

Following Maldonado and Gonzalez-Vega (2008), we define the variable *Education Gap* as the difference in terms of years between the child's highest level of education achieved and his/her expected level of education (according to the age). The expected level of education (*Expected Education*) is then equal to *ChildAge-6*. [30] So we define:

$$Education\ gap = max\{0,\ Expected\ Education - Achieved\ Education\}$$

According to this measure, for example, a child who have attended the school up to the secondary school (without exiting in the past) shows an *Education Gap* equal to 0 at time *t*. In contrast, if he/she did not attended the school, *Education Gap* is exactly equal to *Expected Education* according to the age. If, instead, he/she had problems like late entry, repetitions, desertion, etc. the gap is a positive number. As it is evident from its definition the gap is also able to capture whether a child attended the school continuously in the past and thus takes into account his/her cumulated performance.

By replacing the dependent dummy variable $School_{ijt}$ with $EducationGap_{ijt}$ we re-estimate the baseline child-fixed effects model (eq.1, columns 7-10 in table 4) with the following equation:

$$\begin{aligned} EducationGap_{ijt} = {} & \alpha_0 + \alpha_1 NChildren_{jt} + \alpha_2 JobExperience_{jt} + \sum_{m=3}^{7} \alpha_m ParentageCohorts_{jt} + \\ & + \alpha_8 RespEducation_{jt} + \alpha_9 PartnerEducation_{jt} + \alpha_{10} PreAffTrend_{jt} + \\ & + \alpha_{11} Male_{ijt} + \alpha_{12} Childage_{ijt} + \alpha_{13} AffilYears_{jt} + \sum_{l} \alpha_l DYears_l + \upsilon_i + \varepsilon_{ijt} \end{aligned} \quad (5)$$

Estimations are repeated also with pooled OLS. In both cases results are consistent with what we have found so far and robust to sample split.

Specifically, MF-affiliation years *per se* make child's education gap lager but the effect is reversed when considering *either* children living in S. Brigida (*AffilYears*Sbrigida*) *or* with children living more distant from the school (*AffilYears*Distant*) (table 6, col. 5-8). Again, only the children living in S. Brigida who live more distant from the school (*AffilYears*Sbrigida*Distant*) seem to benefit more from progressive affiliation to microfinance (Table 7, columns 5-8).

[30] We consider in our panel only children aged 6 to 18.

6. Conclusions

The boom of microfinance around the world and the magic aura created around the same "microfinance" concept in a framework of asymmetric information and lack of uniformly acknowledged standards, creates a situation in which highly heterogeneous financial institutions have interest in using the concept in order to capture financial resources. This reduces the self explanatory power of the "microfinance" term and makes all the more urgent an evaluation with impact studies of different microfinance experiences around the world.

One of the most debated questions in this empirical literature is whether microfinance really promotes wellbeing of borrowers and of their families or traps them into a condition of financial dependence. A direction which may tell us whether there is an effective process of increase in wellbeing comes from the answer to the question on whether the bank-microfinance borrower relationship dynamically raises the likelihood of child schooling.

In our paper we propose an original methodology to perform this type of impact study which may overcome important and common limits in these types of analysis (the impossibility of evaluating with a randomized experiment the impact of an already operating organization, the difficulty of collecting long time series on treatment and control samples). In this respect, the combination of a retrospective panel approach with tests on structural break between pre and post-treatment trends, joined with techniques allowing us to minimize selection and survivorship bias, provides a robust methodology to analyse the dynamics of the bank-borrower relationship on child schooling.

The additional advantage we have in our empirical analysis is to address this question on individual data of borrowers from a microfinance organization which has an officially neutral stance toward child education.

Our findings are mixed and show that the effect is robust and significant only in the district with relatively higher standard of living and for children living at a relatively higher distance from school. Our conclusion is that, in the specific case, microfinance generates positive effects on child schooling only when parent income is above a certain threshold so that the Basu and Van (1998) luxury axiom applies and, specifically, for household in the higher standard of living and more productive area who live at a relatively higher distance from the school. The combination of these findings suggests that microfinance effect depend on income and schooling costs. The bank-borrower relationship may provide additional resources which compensate transport costs for families which are more distant from schools but is ineffective (or even harmful) if the level of income remains nonetheless below the threshold of income under which parents are forced not to send children to school by necessity.

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Table 1. Summary statistics of Socio-Demographic and Economic Variables (Whole Sample)

Variable	*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*	
Respondent's Age	361	43.19114	0.6708767	41.87181	44.51047
Household Income	361	4096.097	259.0923	3586.572	4605.622
Household Food expenditure	361	38.85286	1.585422	35.735	41.97071
Total Productivity	361	17.3678	1.189418	15.02872	19.70688
Productivity from I activity (Respondent)	361	11.06951	0.9987779	9.105338	13.03368
Productivity from II activity (Respondent)	361	2.226235	0.4532565	1.334872	3.117598
Productivity from I activity (Partner)	361	4.04512	0.3502009	3.356423	4.733816
Productivity from II activity (Partner)	361	0.0269314	0.0206987	-.0137742	.0676369
Job Experience (years)	361	8.063712	0.4585132	7.162011	8.965413
Savings/month	361	186.0295	27.65336	131.6471	240.412
Distance from main road	361	1.285319	0.1369268	1.016042	1.554595
N. of persons in the house	361	4.224377	0.1021779	4.023436	4.425317
N.of children	361	2.99169	0.1123689	2.770708	3.212672
Schooling years (Respondent)	361	8.430748	0.1636916	8.108836	8.75266
Schooling years (Partner)	361	5.587258	0.2370289	5.121122	6.053393
Credit cycle	361	6.614958	0.457248	5.715746	7.514171
Total amount of last microcredit received	209	1086.158	44.76348	997.9096	1174.406
Duration of the microcredit (weeks)	209	10.85167	0.2203321	10.4173	11.28604

Variable legend: see Table A4.

Table 2. Summary statistics of Socio-Demographic and Economic Variables by Group

	Eligible non-participants					Clients					Drop-outs				
Variable	*Obs*	*Mean*	*Std. Err.*	*[95% Conf.*	*Interval]*	*Obs*	*Mean*	*Std. Err.*	*[95% Conf.*	*Interval]*	*Obs*	*Mean*	*Std. Err.*	*[95% Conf.*	*Interval]*
Respondent's Age	152	43.68421	1.104722	41.5015	45.86692	150	42.53333	0.9579838	40.64034	44.42632	59	43.59322	1.697304	40.1957	46.99074
Household Income	152	3662.599	462.1428	2749.497	4575.7	150	4982.687	387.5127	4216.956	5748.417	59	2958.864	266.5228	2425.361	3492.368
Household Food expenditure	152	42.29793	3.249835	35.87691	48.71895	150	35.89159	1.725943	32.4811	39.30207	59	37.50605	2.055087	33.39235	41.61976
Total Productivity	152	15.79351	2.223757	11.39981	20.18721	150	20.60705	1.636741	17.37283	23.84127	59	13.1882	1.480573	10.22451	16.15189
Productivity from I activity (Respondent)	152	10.34208	2.111818	6.169552	14.51461	150	12.75111	0.9573707	10.85933	14.64288	59	8.668322	1.318832	6.028393	11.30825
Productivity from II activity (Respondent)	152	2.131734	0.5867983	.9723387	3.29113	150	2.92921	0.9037184	1.14345	4.714969	59	0.6824724	0.3089192	.0641034	1.300841
Productivity from I activity (Partner)	152	3.319697	0.4336696	2.462853	4.176541	150	4.861917	0.6576364	3.562419	6.161415	59	3.837402	0.719836	2.396494	5.278311
Productivity from II activity (Partner)	152					150	0.0648148	0.0497471	-.0334861	.1631158	59				
Job Experience (years)	152	7.447368	0.684113	6.095699	8.799038	147	9.390476	0.7362667	7.935359	10.84559	50	7.972	1.253365	5.45327	10.49073
N. of temporary employees	152	0.0263158	0.0130265	.000578	.0520536	150	0.06	0.0254358	.0097385	.1102615	59				
Savings/month	152	78.48684	25.43209	28.23815	128.7355	150	313.8444	57.65782	199.9118	427.7771	59	138.1356	41.49351	55.07732	221.1939
N. of persons in the house	150	4.013333	0.1608108	3.695569	4.331098	150	4.44	0.1529662	4.137737	4.742263	59	4.355932	0.2450715	3.865368	4.846496
N.of children	152	2.519737	0.1600503	2.20351	2.835964	150	3.253333	0.169797	2.917812	3.588854	59	3.542373	0.3182745	2.905277	4.179469
Schooling years (Respondent)	150	8.9	0.2614278	8.383415	9.416585	150	8.403333	0.2370445	7.93493	8.871736	59	7.59322	0.3753966	6.841782	8.344658
Schooling years (Partner)	152	5.828947	0.3903659	5.057663	6.600232	150	5.28	0.3360675	4.615926	5.944074	59	5.745763	0.6056394	4.533444	6.958082
Credit cycle						150	15.76	0.4911458	14.78949	16.73051					
Total amount of last microcredit received						150	1209.513	52.15598	1106.452	1312.574					
Duration of the microcredit (weeks)						150	10.84	0.1938841	10.45688	11.22312					

Variable legend: see Table A4.

Table 3. Summary statistics of Socio-Demographic and Economic Variables by Geographic Area

	MITRE					S. BRIGIDA					VILLA DE MAYO				
Variable	*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*		*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*		*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*	
Respondent's Age	147	43.83673	12.53436	41.79356	45.87991	165	41.97576	12.57269	40.04312	43.9084	49	45.34694	13.76673	41.39267	49.30121
Household Income	147	3750.075	2479.137	3345.96	4154.19	165	4666.333	6627.107	3647.632	5685.034	49	3213.98	3274.602	2273.404	4154.555
Household Food expenditure	147	37.36071	21.43412	33.86681	40.8546	165	41.17489	38.61064	35.23977	47.11002	49	35.5102	16.66738	30.72277	40.29763
Total Productivity	147	15.45767	12.56126	13.4101	17.50523	165	20.08081	30.12544	15.45001	24.71161	49	13.96254	14.18911	9.886948	18.03812
Productivity from I activity (Respondent)	147	10.06212	9.299722	8.546205	11.57803	165	12.8035	26.12121	8.788217	16.81878	49	8.252741	9.221309	5.604072	10.90141
Productivity from II activity (Respondent)	147	1.91264	6.073638	.922599	2.902681	165	2.512452	10.75937	.8585502	4.166353	49	2.203231	6.922279	.2149207	4.191542
Productivity from I activity (Partner)	147	3.482909	5.809828	2.535871	4.429948	165	4.705933	7.757082	3.513536	5.898331	49	3.506563	4.585527	2.189446	4.823681
Productivity from II activity (Partner)	147					165	0.0589226	0.5810446	-.0303941	.1482392	49				
Job Experience (years)	147	8.655782	8.446261	7.278989	10.03258	165	7.696364	9.041152	6.306582	9.086145	49	7.52449	8.431971	5.102545	9.946435
Savings/month	147	178.9116	407.493	112.4877	245.3355	165	238.9495	667.5099	136.3417	341.5573	49	29.18367	90.11189	3.300516	55.06683
N. of persons in the house	147	4.340136	1.981063	4.01721	4.663062	165	4.218182	1.834868	3.936131	4.500233	49	3.897959	2.162607	3.276786	4.519133
N.of children	147	3.244898	2.069319	2.907586	3.58221	165	2.951515	2.188679	2.615077	3.287953	49	2.367347	2.048311	1.779003	2.955691
Schooling years (Respondent)	147	8.183673	3.194645	7.662926	8.704421	165	8.433333	2.811417	8.00117	8.865497	49	9.163265	3.710048	8.097615	10.22892
Schooling years (Partner)	147	5.306122	4.344123	4.598003	6.014242	165	5.569697	4.447833	4.885988	6.253406	49	6.489796	5.103604	5.02387	7.955722
Credit cycle	147	9.088435	9.529281	7.535103	10.64177	165	6.375758	8.137801	5.124837	7.626678	49				
Total amount of last microcredit received	106	1226.038	678.1511	1095.434	1356.642	103	942.2039	582.5151	828.3574	1056.05	0				
Duration of the microcredit (weeks)	106	11.03774	3.488551	10.36588	11.70959	103	10.66019	2.844166	10.10433	11.21606	0				

Variable legend: see Table A4.

Table 4. The effect the length of borrowing relationship with the MF institutions on child schooling (fixed-effects estimates)

Dep. Var: *School*	(1) Whole Sample	(2) Clients&Drops	(3) Whole Sample	(4) Clients&Drops	(5) Whole Sample	(6) Clients&Drops	(7) Whole Sample	(8) Clients&Drops	(9) Whole Sample	(10) Clients&Drops
Parentage 29-33	3.463***	3.329***	3.251***	3.123***	2.993***	3.014***	2.705***	2.759***	2.806***	2.671***
	(0.965)	(0.977)	(0.963)	(0.975)	(0.978)	(0.992)	(0.984)	(0.999)	(0.998)	(1.009)
Parentage 34-38	3.974***	3.660***	3.737***	3.403***	3.646***	3.473***	3.281***	3.104**	3.232**	2.812**
	(1.210)	(1.314)	(1.216)	(1.320)	(1.219)	(1.325)	(1.240)	(1.351)	(1.258)	(1.372)
Parentage 39-43	4.055***	3.645**	3.780***	3.354**	3.598**	3.304**	3.196**	2.914*	3.328**	2.832*
	(1.431)	(1.533)	(1.443)	(1.546)	(1.452)	(1.558)	(1.477)	(1.587)	(1.480)	(1.590)
Parentage 44-48	2.254	1.825	2.128	1.641	1.408	1.182	1.213	0.952	1.711	1.146
	(1.907)	(1.993)	(1.893)	(1.988)	(2.024)	(2.098)	(1.997)	(2.086)	(1.916)	(2.021)
Parentage >48	-11.40	-12.69	-11.64	-12.85	-11.96	-12.83	-12.66	-13.24	-12.46	-13.49
	(809.4)	(1175)	(836.7)	(1117)	(718.3)	(957.0)	(839.5)	(918.1)	(894.0)	(965.4)
Childage	-4.141***	-3.917***	-4.203***	-4.016***	-4.076***	-3.896***	-4.152***	-3.996***	-4.147***	-3.968***
	(0.421)	(0.450)	(0.429)	(0.463)	(0.411)	(0.444)	(0.422)	(0.458)	(0.428)	(0.461)
PreAfftTend	-0.473	0.0236	-0.485	0.00924	-0.722**	-0.0967	-0.748**	-0.126	-0.519*	-0.0128
	(0.311)	(0.188)	(0.309)	(0.195)	(0.344)	(0.261)	(0.346)	(0.281)	(0.311)	(0.205)
JobExperience	1.355***	1.669***	1.361***	1.668***	1.094**	1.502***	1.094**	1.491***	1.312***	1.623***
	(0.408)	(0.395)	(0.408)	(0.400)	(0.429)	(0.429)	(0.433)	(0.440)	(0.412)	(0.405)
NChildren	1.895***	1.569***	2.016***	1.691***	1.958***	1.555***	2.086***	1.694***	2.070***	1.745***
	(0.454)	(0.504)	(0.465)	(0.521)	(0.458)	(0.512)	(0.469)	(0.527)	(0.472)	(0.533)
AffilYears	**0.537**	**0.147**	**0.256**	**-0.0400**	**-0.358**	**-0.385**	**-0.454**	**-0.476**	**0.154**	**-0.106**
	(0.331)	**(0.275)**	**(0.368)**	**(0.290)**	**(0.354)**	**(0.304)**	**(0.336)**	**(0.297)**	**(0.357)**	**(0.280)**
AffilYears*Sbrigida			**0.882****	**0.815****			**0.769****	**0.740***		
			(0.424)	**(0.406)**			**(0.387)**	**(0.385)**		
AffilYears*Distant					**1.485*****	**1.024****	**1.415*****	**0.991****		
					(0.479)	**(0.417)**	**(0.471)**	**(0.417)**		
AffilYears*Sbrigida*Distant									**1.517*****	**1.447*****
									(0.428)	**(0.406)**
Year Dummies	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*5158*	*3600*	*5158*	*3600*	*5158*	*3600*	*5158*	*3600*	*5158*	*3600*
Number of child fixed effects	*509*	*348*	*509*	*348*	*509*	*348*	*509*	*348*	*509*	*348*
AIC	*357.8983*	*307.8569*	*355.7026*	*306.0516*	*349.6724*	*303.3348*	*347.9264*	*301.8585*	*348.1513*	*298.572*
BIC	*482.3161*	*425.4419*	*486.6687*	*429.8254*	*480.6384*	*427.1086*	*485.4408*	*431.8209*	*479.1174*	*422.3457*
Log-likelihood	*-159.9*	*-134.9*	*-157.9*	*-133.0*	*-154.8*	*-131.7*	*-153.0*	*-129.9*	*-154.1*	*-129.3*

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. The effect the length of borrowing relationship with the MF institutions on child schooling (pooled logit estimates)

Dep. Var.: *School*	(1) whole sample	(2) clients&drops	(3) whole sample	(4) clients&drops	(5) whole sample	(6) clients&drops	(7) whole sample	(8) clients&drops	(9) whole sample	(10) clients&drops
Parentage	-0.0267**	-0.0395***	-0.0271**	-0.0402***	-0.0265**	-0.0395***	-0.0270**	-0.0405***	-0.0270**	-0.0400***
	(0.0121)	(0.0148)	(0.0121)	(0.0148)	(0.0121)	(0.0148)	(0.0121)	(0.0149)	(0.0121)	(0.0148)
Childage	-0.525***	-0.525***	-0.526***	-0.527***	-0.525***	-0.525***	-0.526***	-0.527***	-0.526***	-0.527***
	(0.0297)	(0.0374)	(0.0297)	(0.0375)	(0.0298)	(0.0375)	(0.0298)	(0.0376)	(0.0297)	(0.0374)
Male	0.366***	0.278	0.365**	0.279	0.379***	0.294*	0.379***	0.294*	0.370***	0.287*
	(0.142)	(0.173)	(0.142)	(0.174)	(0.142)	(0.173)	(0.142)	(0.174)	(0.142)	(0.173)
RespEducation	0.151***	0.158***	0.150***	0.157***	0.156***	0.163***	0.156***	0.162***	0.150***	0.156***
	(0.0373)	(0.0466)	(0.0373)	(0.0466)	(0.0379)	(0.0475)	(0.0379)	(0.0475)	(0.0373)	(0.0466)
PartnerEducation	0.0552***	0.0563**	0.0540***	0.0537**	0.0543***	0.0561**	0.0529***	0.0533**	0.0532***	0.0528**
	(0.0184)	(0.0230)	(0.0184)	(0.0230)	(0.0184)	(0.0230)	(0.0185)	(0.0230)	(0.0185)	(0.0230)
PreAfftTend	-0.0471**	-0.0175	-0.0479**	-0.0182	-0.0469**	-0.0173	-0.0476**	-0.0179	-0.0482**	-0.0185
	(0.0219)	(0.0242)	(0.0219)	(0.0242)	(0.0218)	(0.0241)	(0.0218)	(0.0241)	(0.0219)	(0.0241)
JobExperience	0.0362***	0.0283**	0.0370***	0.0294**	0.0359***	0.0281**	0.0367***	0.0292**	0.0373***	0.0298**
	(0.0105)	(0.0121)	(0.0106)	(0.0122)	(0.0105)	(0.0121)	(0.0106)	(0.0122)	(0.0106)	(0.0122)
NChildren	-0.0826***	-0.0496	-0.0801**	-0.0448	-0.0826***	-0.0503	-0.0798**	-0.0454	-0.0780**	-0.0422
	(0.0319)	(0.0385)	(0.0320)	(0.0388)	(0.0319)	(0.0385)	(0.0320)	(0.0387)	(0.0321)	(0.0389)
AffilYears	**-0.0170**	**0.0321**	**-0.0480**	**0.00119**	**-0.193****	**-0.135**	**-0.241*****	**-0.179***	**-0.0504**	**-0.00105**
	(0.0489)	**(0.0555)**	**(0.0531)**	**(0.0582)**	**(0.0856)**	**(0.0920)**	**(0.0876)**	**(0.0927)**	**(0.0522)**	**(0.0581)**
AffilYears*Sbrigida			**0.157***	**0.190****			**0.181****	**0.207****		
			(0.0937)	**(0.0945)**			**(0.0917)**	**(0.0934)**		
AffilYears*Distant					**0.229****	**0.207****	**0.245*****	**0.220****		
					(0.0905)	**(0.0921)**	**(0.0896)**	**(0.0910)**		
AffilYears*Sbrigida*Distant									**0.242****	**0.260****
									(0.103)	**(0.105)**
Year Dummies	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*
AIC	*4623.855*	*3201.536*	*4619.987*	*3195.302*	*4611.723*	*3192.431*	*4606.229*	*3185.046*	*4616.327*	*3192.895*
BIC	*4755.226*	*3325.195*	*4758.271*	*3325.469*	*4750.007*	*3322.598*	*4751.428*	*3321.721*	*4754.611*	*3323.062*
Log-likelihood	*-2293*	*-1582*	*-2290*	*-1578*	*-2286*	*-1576*	*-2282*	*-1572*	*-2288*	*-1576*

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Robustness checks (specifications n. 7 and 8)

Model:	a) RANDOM EFFECTS		b) MULTILEVEL		c) FIXED EFFECTS		d) POOLED OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var:	*School*	*School*	*School*	*School*	*Education Gap*	*Education Gap*	*Education Gap*	*Education Gap*
	Whole Sample	Clients&Drops	Whole Sample	Clients&Drops	Whole Sample	Clients&Drops	Whole Sample	Clients&Drops
Parentage 29-33	1.803**	1.943**	1.803**	1.925**	-1.005***	-1.063***	-0.705***	-0.766***
	(0.749)	(0.807)	(0.734)	(0.791)	(0.148)	(0.187)	(0.0993)	(0.121)
Parentage 34-38	1.229	1.604*	1.205	1.503*	-1.156***	-1.378***	-0.723***	-0.866***
	(0.829)	(0.908)	(0.821)	(0.899)	(0.241)	(0.298)	(0.121)	(0.152)
Parentage 39-43	1.149	1.335	1.111	1.223	-0.875***	-0.997**	-0.567***	-0.658***
	(0.873)	(0.960)	(0.871)	(0.961)	(0.313)	(0.393)	(0.131)	(0.170)
Parentage 44-48	0.220	0.203	0.313	0.134	-0.435	-0.578	-0.405***	-0.501**
	(0.909)	(1.005)	(0.924)	(1.030)	(0.400)	(0.497)	(0.157)	(0.211)
Parentage >48	0.143	0.0317	0.452	0.0707	-0.134	-0.153	-0.428**	-0.336
	(0.974)	(1.087)	(1.009)	(1.136)	(0.502)	(0.619)	(0.203)	(0.246)
Male	0.779*	0.596	0.515	0.530			-0.205**	-0.134
	(0.414)	(0.484)	(0.365)	(0.429)			(0.0818)	(0.101)
RespEducation	0.447***	0.418***	0.386***	0.357***			-0.0792***	-0.0920***
	(0.0837)	(0.0967)	(0.114)	(0.134)			(0.0171)	(0.0223)
PartnerEducation	0.124**	0.0761	0.0995	0.0179			-0.0296***	-0.0321**
	(0.0527)	(0.0633)	(0.0725)	(0.0889)			(0.0108)	(0.0133)
Childage	-1.571***	-1.543***	-1.565***	-1.527***	0.979***	1.012***	0.846***	0.873***
	(0.0925)	(0.102)	(0.0938)	(0.103)	(0.0521)	(0.0629)	(0.0131)	(0.0161)
PreAfftTend	-0.0669	-0.00230	-0.0363	0.0132	-0.0280	0.0318	0.0222	0.00790
	(0.0640)	(0.0636)	(0.0674)	(0.0659)	(0.0396)	(0.0388)	(0.0164)	(0.0177)
JobExperience	0.0589*	0.0487	0.0252	0.0347	-0.0650	-0.00152	-0.0218***	-0.0152*
	(0.0303)	(0.0333)	(0.0396)	(0.0430)	(0.0425)	(0.0496)	(0.00795)	(0.00890)
NChildren	-0.193**	-0.0715	-0.149	-0.0178	-0.115	-0.179	0.0540***	0.0374
	(0.0932)	(0.112)	(0.140)	(0.164)	(0.109)	(0.129)	(0.0204)	(0.0254)
AffilYears	**-0.625*** **	**-0.621*** **	**-0.636*** **	**-0.678*** **	**0.312*** **	**0.191***	**0.184*** **	**0.124***
	(0.190)	**(0.194)**	**(0.199)**	**(0.201)**	**(0.0923)**	**(0.0982)**	**(0.0684)**	**(0.0731)**
AffilYears*Sbrigida	**0.496** **	**0.502** **	**0.507** **	**0.506** **	**-0.310*** **	**-0.339*** **	**-0.209** **	**-0.241*** **
	(0.225)	**(0.218)**	**(0.246)**	**(0.236)**	**(0.111)**	**(0.110)**	**(0.0836)**	**(0.0844)**
AffilYears*Distant	**0.618*** **	**0.596*** **	**0.573*** **	**0.564*** **	**-0.252** **	**-0.229** **	**-0.214*** **	**-0.189*** **
	(0.208)	**(0.202)**	**(0.222)**	**(0.213)**	**(0.101)**	**(0.100)**	**(0.0721)**	**(0.0731)**
Year-Dummies	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	20.85***	19.21***	21.05***	19.44***	-6.064***	-6.166***	-5.196***	-5.193***
	(1.541)	(1.671)	(1.719)	(1.903)	(0.434)	(0.536)	(0.237)	(0.295)
RE 1: *child (std.dev.)*	3.276***	3.170***	3.736***	3.596***				
	(0.131)	(0.151)	(0.373)	(0.324)				
RE 2: *family (std.dev.)*			3.734***	3.402***				
			(0.291)	(0.405)				
number of level 1 units	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*
number of level 2 units (child)	*861*	*562*	*861*	*562*	*861*	*562*	*861*	*562*
number of level 3 units (family)			*295*	*176*				
R-squared					*0.578*	*0.602*	*0.613*	*0.629*

(5-6) Child-Clustered Standard errors in parentheses; (7-10) Robust S.E. in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Robustness checks (specifications n. 9 and 10)

Model:	a) RANDOM EFFECTS		b) MULTILEVEL		c) FIXED EFFECTS		d) POOLED OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var:	*School*	*School*	*School*	*School*	*Education Gap*	*Education Gap*	*Education Gap*	*Education Gap*
	Whole Sample	Clients&Drops	Whole Sample	Clients&Drops	Whole Sample	Clients & Drops	Whole Sample	Clients & Drops
Parentage 29-33	1.794**	1.928**	1.823**	1.949**	-0.999***	-1.056***	-0.700***	-0.758***
	(0.751)	(0.812)	(0.740)	(0.803)	(0.148)	(0.187)	(0.0992)	(0.121)
Parentage 34-38	1.185	1.535*	1.198	1.483	-1.143***	-1.360***	-0.714***	-0.850***
	(0.832)	(0.913)	(0.823)	(0.908)	(0.241)	(0.297)	(0.120)	(0.151)
Parentage 39-43	1.137	1.311	1.161	1.239	-0.872***	-0.994**	-0.565***	-0.656***
	(0.876)	(0.966)	(0.873)	(0.971)	(0.312)	(0.392)	(0.130)	(0.168)
Parentage 44-48	0.223	0.195	0.371	0.135	-0.432	-0.570	-0.402**	-0.491**
	(0.912)	(1.011)	(0.925)	(1.038)	(0.399)	(0.496)	(0.156)	(0.210)
Parentage >48	0.111	-0.0203	0.519	0.0676	-0.146	-0.171	-0.426**	-0.342
	(0.977)	(1.093)	(1.010)	(1.145)	(0.501)	(0.617)	(0.202)	(0.244)
Male	0.758*	0.570	0.492	0.494			-0.202**	-0.132
	(0.416)	(0.488)	(0.364)	(0.430)			(0.0819)	(0.102)
RespEducation	0.431***	0.395***	0.402***	0.356***			-0.0767***	-0.0884***
	(0.0839)	(0.0972)	(0.117)	(0.136)			(0.0171)	(0.0223)
PartnerEducation	0.121**	0.0694	0.103	0.0129			-0.0296***	-0.0319**
	(0.0530)	(0.0638)	(0.0736)	(0.0905)			(0.0108)	(0.0133)
Childage	-1.581***	-1.558***	-1.574***	-1.556***	0.980***	1.013***	0.847***	0.874***
	(0.0938)	(0.104)	(0.104)	(0.115)	(0.0521)	(0.0629)	(0.0131)	(0.0161)
PreAfftTend	-0.0705	-0.00587	-0.0358	0.0140	-0.0305	0.0289	0.0229	0.00832
	(0.0644)	(0.0642)	(0.0665)	(0.0659)	(0.0395)	(0.0388)	(0.0164)	(0.0177)
JobExperience	0.0642**	0.0551	0.0287	0.0408	-0.0678	-0.00584	-0.0224***	-0.0157*
	(0.0305)	(0.0336)	(0.0399)	(0.0439)	(0.0425)	(0.0498)	(0.00795)	(0.00892)
NChildren	-0.182*	-0.0521	-0.139	0.00566	-0.120	-0.182	0.0521**	0.0342
	(0.0939)	(0.113)	(0.140)	(0.165)	(0.108)	(0.127)	(0.0205)	(0.0256)
AffilYears	**-0.218***	**-0.223***	**-0.296****	**-0.353****	**0.134***	**0.0208**	**0.0163**	**-0.0316**
	(0.123)	**(0.132)**	**(0.140)**	**(0.149)**	**(0.0702)**	**(0.0770)**	**(0.0434)**	**(0.0479)**
AffilYears*Sbrigida*Distant	**0.767*****	**0.775*****	**0.762****	**0.789*****	**-0.423*****	**-0.425*****	**-0.286*****	**-0.308*****
	(0.268)	**(0.263)**	**(0.301)**	**(0.291)**	**(0.121)**	**(0.119)**	**(0.0978)**	**(0.0978)**
Year-Dummies	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	21.10***	19.56***	20.76***	19.57***	-6.053***	-6.158***	-5.224***	-5.231***
	(1.563)	(1.705)	(1.776)	(1.995)	(0.431)	(0.531)	(0.237)	(0.294)
RE 1: *child (std.dev.)*	3.293***	3.195***	3.638***	3.638***				
	(0.133)	(0.153)	(0.332)	(0.371)				
RE 2: *family (std.dev.)*			3.807***	3.518***				
			(0.394)	(0.431)				
number of level 1 units	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*	*7437*	*4956*
number of level 2 units (child)	*861*	*562*	*861*	*562*	*861*	*562*	*861*	*562*
number of level 3 units (family)			*295*	*176*				
R-squared					*0.613*	*0.629*	*0.578*	*0.601*

(5-6) Child-Clustered Standard errors in parentheses; (7-10) Robust S.E. in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix

Table A1. Summary statistics of Socio-Demographic and Economic Variables by Geographic Area (ONLY CLIENTS)

	MITRE					S. BRIGIDA				
Variable	*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*		*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*	
Respondent's Age	76	44.32895	1.365539	41.60865	47.04924	74	40.68919	1.318134	38.06215	43.31623
Household Income	76	4419.224	300.5372	3820.523	5017.924	74	5561.378	718.9793	4128.455	6994.302
Household Food expenditure	76	36.78415	2.850863	31.10494	42.46336	74	34.9749	1.930336	31.12775	38.82206
Total Productivity	76	16.60968	1.355221	13.90994	19.30942	74	24.71246	2.947885	18.83733	30.58758
Productivity from I activity (Respondent)	76	11.07676	0.9560213	9.172272	12.98126	74	14.4707	1.657645	11.16702	17.77438
Productivity from II activity (Respondent)	76	1.770285	0.5189164	.7365506	2.80402	74	4.119457	1.748307	.6350865	7.603827
Productivity from I activity (Partner)	76	3.762628	0.7150822	2.338111	5.187145	74	5.990917	1.102422	3.793793	8.188041
Productivity from II activity (Partner)	76					74	0.1313814	0.1005943	-.0691029	.3318656
Job Experience (years)	76	10.125	1.042775	8.047686	12.20231	74	8.255405	1.014924	6.232665	10.27815
Savings/month	76	253.9474	53.4963	147.3773	360.5175	74	375.3604	103.113	169.8565	580.8642
N. of persons in the house	76	4.394737	0.236998	3.922613	4.866861	74	4.486486	0.1936069	4.100629	4.872344
N.of children	76	3.421053	0.2479836	2.927044	3.915061	74	3.081081	0.2315033	2.619696	3.542466
Schooling years (Respondent)	76	8.118421	0.3684806	7.38437	8.852472	74	8.695946	0.2947802	8.10845	9.283442
Schooling years (Partner)	76	4.75	0.482728	3.788357	5.711643	74	5.824324	0.4619382	4.903683	6.744966
Credit cycle	76	17.57895	0.5795531	16.42442	18.73348	74	13.89189	0.7411248	12.41483	15.36895
Total amount of last microcredit received	76	1320.395	76.711	1167.579	1473.211	74	1095.635	68.5187	959.0776	1232.193
Duration of the microcredit (weeks)	76	10.96053	0.2982056	10.36647	11.55458	74	10.71622	0.2475502	10.22285	11.20958

Table A2. Summary statistics of Socio-Demographic and Economic Variables by Geographic Area (ONLY ELIGIBLE NON-PARTICIPANTS)

	MITRE					S. BRIGIDA					VILLA DE MAYO				
Variable	*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*		*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*		*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*	
Respondent's Age	40	43.60976	2.181093	39.2016	48.01791	61	42.41935	1.695882	39.02823	45.81048	49	45.34694	1.966676	41.39267	49.30121
Household Income	40	2641.463	304.1126	2026.829	3256.098	61	4692.419	1043.381	2606.05	6778.788	49	3213.98	467.8003	2273.404	4154.555
Household Food expenditure	40	36.32404	2.802678	30.65962	41.98847	61	51.6129	7.399757	36.81617	66.40963	49	35.5102	2.381054	30.72277	40.29763
Total Productivity	40	12.86305	1.901459	9.020058	16.70604	61	19.17845	5.055998	9.068362	29.28855	49	13.96254	2.027015	9.886948	18.03812
Productivity from I activity (Respondent)	40	7.966277	1.211214	5.518323	10.41423	61	13.56442	5.006187	3.553936	23.57491	49	8.252741	1.31733	5.604072	10.90141
Productivity from II activity (Respondent)	40	2.837527	1.461636	.1165488	5.791604	61	1.608494	0.7383399	.132092	3.084896	49	2.203231	0.988897	.2149207	4.191542
Productivity from I activity (Partner)	40	2.059247	0.6687287	.7076955	3.410798	61	4.005537	0.8094253	2.386991	5.624082	49	3.506563	0.6550754	2.189446	4.823681
Productivity from II activity (Partner)	40					61					49				
Job Experience (years)	40	6.990244	1.212173	4.540351	9.440137	61	7.68871	1.139043	5.411052	9.966368	49	7.52449	1.204567	5.102545	9.946435
Savings/month	40	68.29268	4.381.767	20.26613	156.8515	61	124.1935	54.06214	16.08961	232.2975	49	29.18367	12.87313	3.300516	55.06683
N. of persons in the house	40	4.243902	0.3019863	3.633565	4.854239	61	3.822581	0.2471671	3.328339	4.316822	49	3.897959	0.3089439	3.276786	4.519133
N.of children	40	2.853659	0.3095256	2.228084	3.479233	61	2.419355	0.2428767	1.933693	2.905017	49	2.367347	0.2926159	1.779003	2.955691
Schooling years (Respondent)	40	8.292683	0.5275853	7.226393	9.358973	61	8.806452	0.3812354	8.044124	9.568779	49	9.163265	0.5300068	8.097615	10.22892
Schooling years (Partner)	40	5.512195	0.6915183	4.114585	6.909806	61	5.516129	0.6152229	4.285915	6.746343	49	6.489796	0.7290862	5.02387	7.955722

Table A3. Summary statistics of Socio-Demographic and Economic Variables by Geographic Area (DROP-OUTS)

	MITRE					S. BRIGIDA				
Variable	*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*		*Obs*	*Mean*	*Std. Dev.*	*[95% Conf. Interval]*	
Respondent's Age	77	44.24675	1.350192	41.55761	46.93589	75	40.73333	1.301189	38.14066	43.32601
Household Income	77	4394.299	297.6539	3801.47	4987.128	75	5523.227	710.3534	4107.817	6938.637
Household Food expenditure	77	36.56617	2.822026	30.94562	42.18672	75	35.30857	1.933434	31.45612	39.16102
Total Productivity	77	16.51422	1.340907	13.84357	19.18487	75	24.48296	2.917355	18.67	30.29592
Productivity from I activity (Respondent)	77	11.05316	0.943819	9.173382	12.93294	75	14.31109	1.643164	11.03702	17.58517
Productivity from II activity (Respondent)	77	1.747294	0.5126487	.726266	2.768323	75	4.064531	1.725713	.625973	7.503089
Productivity from I activity (Partner)	77	3.713763	0.707424	2.304806	5.12272	75	5.977705	1.087704	3.810407	8.145002
Productivity from II activity (Partner)	77					75	0.1296296	0.0992595	-.0681492	.3274085
Job Experience (years)	77	10.05844	1.031293	8.004443	12.11244	75	8.372	1.008066	6.363385	10.38061
Savings/month	77	250.6494	52.89987	145.2901	356.0086	75	370.3556	101.8519	167.4112	573.2999
N. of persons in the house	77	4.441558	0.2385402	3.966465	4.916652	75	4.573333	0.2098248	4.155248	4.991418
N.of children	77	3.376623	0.2487419	2.881211	3.872036	75	3.093333	0.2287241	2.637591	3.549076
Schooling years (Respondent)	77	8.012987	0.3786391	7.258862	8.767112	75	8.713333	0.2913425	8.132821	9.293846
Schooling years (Partner)	77	4.688312	0.4803948	3.731523	5.645101	75	5.786667	0.4572906	4.875495	6.697838
Credit cycle	77	17.35065	0.6158552	16.12407	18.57723	75	13.70667	0.7542727	12.20375	15.20959
Total amount of last microcredit received	77	1320.395	76.711	1167.579	1473.211	75	1095.635	68.5187	959.0776	1232.193
Duration of the microcredit (weeks)	77	10.96053	0.2982056	10.36647	11.55458	75	10.71622	0.2475502	10.22285	11.20958

Table A4. Variable Legend

Variable	*Description*
Respondent's Age (Parentage in tab. 5)	Respondents' Age
Household Income	Total monthly family income in pesos (monthly income from all the respondent's activities + monthly income from all the activities of respondent's partner + contributions by other members living in the household).
Household Food expenditure	Daily family food expenditure in pesos
Total Productivity	Monthly income from each activities of each family members per hour worked (in pesos).
Productivity from I activity (Respondent)	Monthly income from the respondent's main activity per hour worked (in pesos).
Productivity from II activity (Respondent)	Monthly income from the respondent's secondary activity (if any) per hour worked (in pesos).
Productivity from I activity (Partner)	Monthly income from the partner's main activity per hour worked (in pesos).
Productivity from II activity (Partner)	Monthly income from the partner's secondary activity per hour worked (in pesos).
Job Experience (years)	Respondent's years of experience in the main activity
Savings/month	Respondent's monthly savings (in pesos)
N. of persons in the house	Number of household members
N.of children (*NChildren*)	Total number of children in the household
Schooling years (Respondent) (*RespEducation*)	Respondent's years of education
Schooling years (Partner) (*PartnerEducation*)	Years of education of the respondent's partner
Credit cycle	Cycle of loan received from the MFI (credit seniority)
Total amount of last microcredit received	Overall amount of the loan received (in pesos)
Duration of the microcredit (weeks)	Length of the loan (weeks).
Male	Dummy = 1 if child is male
Parentage (tab. 4,6,7)	Respondent's age cohort dummies: years 29-33; 34-38; 39-43; 44-48; >48 (omitted benchmark is 0-29)
Childage	Child's age (years)
PreAfftTend	Trend variable measuring the number of years before becoming MFI-borrower
AffilYears	Years of uninterrupted lending relationship with the MFI (affiliation years)
Sbrigida	Dummy = 1 if respondent lives in the village of S. Brigida.
Distant	Dummy = 1 if child lives above the median distance from the school (measured in *cuadras*: 1 km = 12 cuadras)
Year-Dummies	Time dummies (1989 is the omitted benchmark)